TRANSACTIONS
OF
THE OPTICAL SOCIETY.

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Ordinary Meeting, 11th October, 1917.

Held at the Imperial College of Science and Technology, South Kensington.

Professor F. J. CHESHIRE, F.R.M.S., President, in the Chair.

The Minutes of the previous Ordinary Meeting were read, confirmed and signed.

The following were balloted for and duly elected to the Society :—

As Members.

J. Victor Armstrong, Consulting Engineer, Liverpool.
Rowland Arnison, Messrs. Short and Mason, London.
Selig Brodetsky, Lecturer in Applied Mathematics, University of Bristol.
Francis E. Collinson, Managing Director, Messrs. Short and Mason, London.
Charles Flint, F.S.M.C., F.I.O., Ophthalmic Optician, London.
J. Guild, A.R.C.S., D.I.C., F.R.A.S., National Physical Laboratory, Teddington.
Arthur Joseph Hughes, Marine Optician, London.
George Paxton, Messrs. Curry and Paxton, London.
Ernest Rheinberg, London.
Sydney George Starling, A.R.C.Sc., B.Sc., Head of Physical Department, Municipal Technical Institute, West Ham.
Percy R. Warman, Optician, St. Leonards-on-Sea.

It was announced that the undermentioned candidates had been approved by the Council for the ballot. The name of the proposer is appended :—

For the class of "Member."

John Spence Anderson, M.A., B.Sc., Ph.D., Teddington (W. Shackleton).
H. R. Broadhurst, London (J. Pizzala).
Gilbert S. Dey, New York (W. Shackleton).
Frank Theodore Gerstenberg, B.Sc., Glasgow (F. J. Cheshire).
John Wilkinson Purkis, London (J. Rheinberg).
Albert Edward Rowlings, London (S. D. Chalmers).

For the class of "Student."

Herbert Charles Briers, London (F. Twyman),

The Grading of Carborundum for Optical Purposes.

By JAMES WEIR FRENCH, B.Sc.

Read by Dr. R. S. Clay (in the absence of the Author), and discussed 11th October, 1917.

In most optical factories it is customary to use a coarse grade of carborundum, say 80 mesh, for the preliminary grinding, and to use several fine grades of emery, say, three, fifteen, or forty minute emery, for the fine grinding or smoothing operations prior to the polishing operation.

Under this system of working it is necessary to separate with great care the carborundum and emery operations. Contamination of the carborundum with equal or finer grades of emery is of small importance, but on the other hand contamination of the emery with even a considerably finer grade of carborundum may cause serious trouble. In any case care must be taken to prevent the contamination of one grade with a considerably coarser one of the same type, but when emery and carborundum are employed together, there is involved the additional labour of keeping the carborundum clear of the emery.

By the use of a single type of abrasive throughout the optical shops, a considerable simplification of the organisation can be effected.

As carborundum is grade for grade at least four times as effective as emery, the former abrasive is the one that is recommended for adoption, but the choice of carborundum is justifiable also on political grounds. Emery is a natural material not indigenous to this country; it is conceivable that in time of war our supplies of emery might be interrupted. Carborundum, on the other hand, is an artificial substance, the manufacture of which in this country could be quickly increased when required in time of war. It is important that the optical manufacturer should not lose sight of war conditions when organising his methods, because the essential character of the optical industry in time of war is beyond question. Without discounting in any way the importance of the man behind the barrage or the gun, there is no doubt that modern warfare is dependent upon the perfection of the appliances employed, and very many of these appliances are optical or involve optical gear.

As ranges increase in the future, the need for many kinds of optical appliances will become even greater than at present, and in the interests

of the country the optical industry should keep well in view possible future war conditions.

Carborundum (Si C) is a compound of carbon and silicon resulting from the fusion of carbonaceous materials such as coke or charcoal with sand in the electric furnace at a temperature of about 2,000° C. For fluxing purposes common salt is generally added to the batch, and sawdust is employed to give porosity to the mass during the action. The crystalline product is separated and treated in various ways to remove impurities. Free silica may be removed by treatment with hydrofluoric acid, but the carborundum as supplied generally contains a proportion of undissolved free silica.

Although carborundum is ordinarily insoluble even in hydrofluoric acid, it is readily soluble in various reagents after fusion with glasses containing alkalies. Its specific gravity is 3.1, which, it should be noted, is about the mean value of the density of the glasses most commonly used.

The hardness of carborundum is stated to be about 9, diamond being 10, and emerald 8. It should be noted that the interval between 9 and 10 on Mohs' scale is considerably greater than the interval from 8 to 9. No very precise meaning can be attached to the word "hardness," and the figures quoted can only be very approximate. A substance that will scratch another is said to be harder. Resistance to compression, shear and cleavage are accordingly involved in the term. Carborundum is harder than emery, but its relative hardness is emphasised by the ease with which emery cleaves.

Crystals of carborundum have usually a green colour which varies in intensity according to the amount of iron impurity present. When the carborundum is ground to, say, 80 mesh size, the grains under the microscope are often indistinguishable from fragments of glass, but some of the grains appear green and others blue, due to the local distribution of impurities in the original mass.

There must be some very definite reasons why only the coarsest grades of carborundum are commonly employed in optical glass grinding. One of these reasons is doubtless the excessive cost of the finer grades. In Fig. 1 curve A shows the relation of the present cost in pence to the grade for carborundum, and curve B the same relationship for emery. So far as emery is concerned, the price hardly increases with the fineness of the grade; the curve is practically a horizontal line. But the carborundum curve is quite different. For the first series of coarse grades the price is constant; then the price rises regularly and rapidly up to a maximum; thereafter the price is maintained constant. Evidently the scale of prices is determined by commercial considerations more than by the possibly greater cost of grinding and grading carborundum, as compared with emery. If fine grades of carborundum were obtainable

at a reasonably low price, the use of carborundum should rapidly increase in proportion to the use of emery, and fortunately the process of washing and grading carborundum up to a certain stage is not an expensive one. In the case of a plant dealing with over one and a-half tons of worked abrasive per week, the total cost of washing and grading to a high degree of fineness does not exceed 0.7 pence per pound. For smaller weekly outputs the cost will no doubt be somewhat greater, but it cannot be of serious amount.

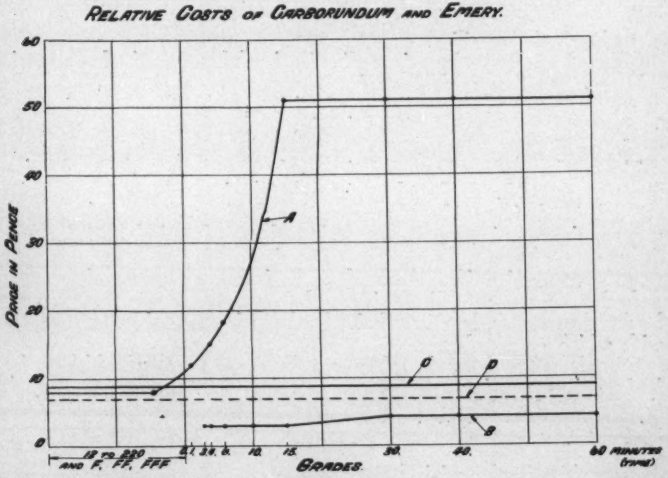


FIG. 1.

Curve C accordingly represents the cost of the carborundum after grading in the workshop, and it will be evident that its general use is now quite practicable so far as cost is concerned.

Since for equal weights the volumes of carborundum and emery are in the ratio of 4 : 3, the curve D shows the cost of equal volumes of the graded carborundum and the emery. As the carborundum has several times the abrasive value of the emery, it will be evident that the carborundum is the more economical material.

From the practical point of view the nature of the abraded surface is the first question that will probably arise. It may be thought that the

action of the carborundum will be too drastic, that the formation of deep pits in the forming operations may increase the subsequent time required for smoothing, and that the quality of the smoothed surface may be such as to increase the length of the polishing process.

If the surface irregularities were of cone formation, as is believed by some to be the case, then the use of a powerful abrasive might be expected to give rise to deeper fissures. It is not the purpose of this paper to discuss the nature of abraded surfaces or the conditions that give rise to cone, pot-lid, conchoidal and cleavage fractures. These interesting questions may be left for separate consideration.

From practical experience in the sole use of carborundum, extending over a considerable period of time, the writer has not found any serious abrasion difficulties in the use of that material as compared with the use of emery, provided a suitable sequence of grades is employed. Thus, if the abrasive is separated by equal steps into eight grades, No. 1 corresponding with ordinary commercial 80-mesh corborundum, and No. 8 corresponding in effect with thirty minute emery, then the forming may be done in, say, two stages with No. 1 and No. 3 grades, and the smoothing in one stage with No. 8.

For the attainment of the above-mentioned results it is essential to instal in association with the workshop efficient plant for the grading of the abrasive, and in the design of such plant several interesting questions arise.

So far as grades 1 to 4 are concerned, the separation is an easy matter. As the fineness increases various difficulties arise, and to obtain grades finer than No. 8 is an extremely difficult matter, except by the slow and cumbersome method of simple levigation in still water or other means that have yet to be practically developed. Fortunately, in the case of most small shops, the quantities of the finest abrasives required for ordinary use are comparatively small. A satisfactory method of separating large quantities of very fine abrasive does not appear so far to have been evolved. The difficulties are very great.

It will be assumed that the only fresh carborundum supplied by the store to the shop is of the coarsest and cheapest grade; that this coarse material, together with the coarsest grades from the washing plant, is supplied to the heavy roughing mills for which in most shops sufficient work can generally be found. Thus, one duty of the roughing mills is to break down the coarse abrasive, while performing the coarsest roughing work.

At regular intervals the sludge from the forming and smoothing machines is sent to the washing plant, no attempt being necessarily made to keep the various qualities separate. This sludge is a mud consisting of carborundum of all grades, glass of different qualities and grades, grease, plaster, or other cementing materials, dirt, and quite a considerable

percentage of iron from the grinding tools. It will be evident that the material that has to be dealt with by the optical manufacturer is much more complicated than the fresh carborundum that has to be graded at the abrasive works. Owing to the presence of water and grease, the sludge cakes even when well dried, and to attempt to separate and grade the grains by means of air currents appears to be impracticable. To obtain a uniform current of air is much more difficult than to obtain a uniform flow of water, although even in the case of water the suppression of such eddy currents as will affect the settling of very fine particles of carborundum is by no means easy.

Certain thoroughly dry materials can no doubt be successfully graded by means of air. Sir Henry Bessemer, for instance, employed with the greatest success a current of air for the grading of his bronze powders. His apparatus comprised a long horizontal wooden trunk of uniform section; the powder carried by the current of air was deposited in stages of increasing fineness along the floor of the trunk.

Carborundum sludge is not a suitable material for air treatment. It is more convenient to deal with it by means of a wet process. In all wet processes there are two stages, one the breaking up of the sludge into well separated particles, the other the grading of the separated particles. The isolation of the particles can be very readily effected by thoroughly mixing the sludge with water and by agitating the mixture so as to keep the particles in suspension. For the grading of the suspended particles there are several methods which can be combined with advantage.

It will be interesting to consider first the theoretical aspects of the problem of elutriation, if only to show how much more complex the actual problem is.

According to Sir Gabriel Stokes, a very small sphere settling in still liquid quickly assume a constant velocity proportional to the cross sectional area, as represented by the equation

$$\text{Vel.} = \frac{2}{9}\frac{R^2(S_1 - S_2)g}{n}$$

where

R is the radius of the spherical particle,
S_1 the specific gravity of the particle,
S_2 the specific gravity of the liquid,
g the force of gravity,
n the viscosity constant of the liquid.

For an ideal spherical particle of carborundum equivalent to, say, Grade 80 settling in pure water

R = 0·011 cm.	g = 981
S_1 = 3·1	n at 27°C = 0·0084
S_2 1	

all expressed in centimetre, gramme, second units, and the velocity is then equal to 6 centimetres per second.

But if carborundum particles of Grade 80 are observed falling through water, it will be seen that the velocity of fall is considerably less, being actually about 3.3 centimetres per second. This discrepancy is due to the size of the particles being such that the water becomes turbulent to a serious extent immediately around the falling particles. Under such conditions Stokes' law is not applicable. A considerable departure from the law is to be expected even in the case of the finest No. 8 carborundum, when it is considered that the diameter of the average grain is not much less than 1/100 millimetre, and that the form and surface are very irregular.

On close examination of the falling particles it will be seen that, while some of the grains of quite large size fall with extreme slowness, others actually rise, owing to the grains being suspended from air-bells. Since the specific gravity of carborundum is about 3, an air-bell should be capable of floating a grain of half its volume, provided the adhesion of the grain to the air-bell is sufficiently great, which it appears to be. It should be noted that an air-bell that will just float a particle at a low level will raise it easily at a higher level, owing to the expansion of the air-bell as the head of water decreases.

The adhesion of the particle to the air-bell is probably due to surface tension forces, but it may also to a very slight extent be due to electrical attraction of the oppositely charged air and particles of carborundum, there being a film of non-conducting water between the air and the carborundum.

With regard to the source of the air, the greater portion is no doubt introduced with the sludge, around the particles of which it clings. A smaller portion may be mixed and dissolved with the water, and no doubt the sharp points of the carborundum grains tend to release and collect such dissolved air. Theoretically the air could be eliminated by evacuation, but this is not a practical remedy.

As the amount of air in question is quite considerable, the prolonged flotation of coarse grains over the fine grain zones is a matter of serious practical importance, and it is one of the several difficulties that have to be dealt with in the design of a wet grading plant.

When the carborundum is dropped into the water, the bulk of the material passes through the surface, but an appreciable amount floats. The surface tension of the water is capable of supporting quite large grains which may float for a considerable time over the finer zones, thus constituting an additional practical difficulty. In general, large particles separate themselves by simple settling in water, provided air-bells are eliminated and the eddy currents are suppressed, but when the particles are very fine, grading by simple natural settling in water becomes much more difficult.

Assuming for the moment that Stokes' formula is applicable to carborundum, the velocity of settling should vary directly as the square of the radius. If, therefore, a small sphere were subdivided into a thousand spheres, each individual part would fall theoretically one hundred times more slowly than the original sphere. But it has been suggested that there are other forces involved that prevent the settling of very minute particles suspended in a liquid. When a sphere is subdivided in the manner described, the total surface is increased tenfold, while the total volume remains unaltered. It follows that the surface effects become of increasing importance, and when the particles are very minute the electrical charges on the individual particles may be sufficiently great to prevent them from gathering together and settling. If, however, the electrical charges are neutralised, the particles should gather together and settle.

Fortunately, for the production of the finest grades of carborundum suitable for the smoothing operation, other and simpler methods suffice and it is not necessary to adopt such a process until the question of using the carborundum for polishing arises.

In its most elementary form the grading plant may have the form of a long parallel-sided tank through which a constant velocity flow of water is maintained, the sludge being fed at the inflow end of the tank and the different grades being deposited along the bottom. It is assumed that a constant flow of water can be maintained across any section of the tank, and that the action is not affected by any of the subsidiary conditions already referred to.

If the velocity of flow of the water is v, then in time t a particle will move horizontally through a distance

$$L = v\,t \qquad (1)$$

In the first instance it will be of interest to consider what should be the form of the tank, on the assumption that Stokes' formula can be applied to the settling of a small carborundum grain by a suitable modification of the constant, but it should be clearly understood that in reality this formula is not apparently applicable, as will be more fully explained later.

In the time t the particle would fall through a distance

$$h = \frac{2}{9}\frac{R^2(S_1 - S_2)\,g\,t}{n} \qquad (2)$$

hence

$$t = \frac{9}{2}\frac{h\,n}{R^2(S_1 - S_2)\,g} \qquad (3)$$

Now if t is the time taken by the particle to reach the bottom of the tank of depth h, the horizontal distance through which the particle will have moved before it settles on the bottom can be found by substituting the value of t in the equation $L = vt$, thus

$$L = \frac{9}{2} \frac{v\,h\,n}{R^2 (S_1 - S_2) g} \tag{4}$$

When the unknown variables are L and R, then

$$L \propto \frac{1}{R^2} \tag{5}$$

Thus, if Stokes' law applied, the position along the bottom occupied by any particular grade would vary inversely as the square of the radius of the particles of which the grade consists.

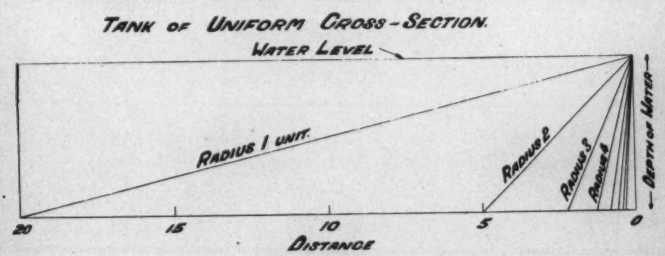


FIG. 2.

This result is indicated diagrammatically in Fig. 2, which shows the curves of settling according to Stokes' law for particles of different diameters. It is assumed that the diameters vary in arithmetical progression. Two interesting features are at once evident. The great majority of the grades are crowded closely together, and on the other hand the finest grade is carried to a distance that is undesirably great.

To separate the coarse grade, a very high initial velocity of horizontal flow would be required, but as this would greatly increase the distance of the finest grade, it would be necessary thereafter to reduce the velocity of flow by increasing the cross sectional area.

In order to determine the best form of grading tank, a series

of experiments was made to determine the velocity of fall in water of various grades of carborundum. Tubes 40 ins. long and of large diameter were employed, and to avoid the production of eddy currents very small quantities were tested at each experiment. Tests were made with distilled water and with fresh Loch Katrine water, but there was no appreciable difference observed. The temperature of the water was 27° C. In all cases the velocity of fall became practically constant after the first few inches. Particles of the coarser grades fell along a very slightly zig-zag path, and viewed by reflected light some of the particles at intervals appeared to lose their equilibrium and to turn over quickly, giving rise to a sudden flash of reflected light. In the case of the fine grades there were slight evidences of eddy currents, but generally the conditions were probably better than could be expected in a well controlled settling tank.

The results which were in some respects unexpected are indicated an Fig. 3. Curve A gives the results calculated from Stokes' law. The abscissae represent the diameters of the spheres in centimetres and the

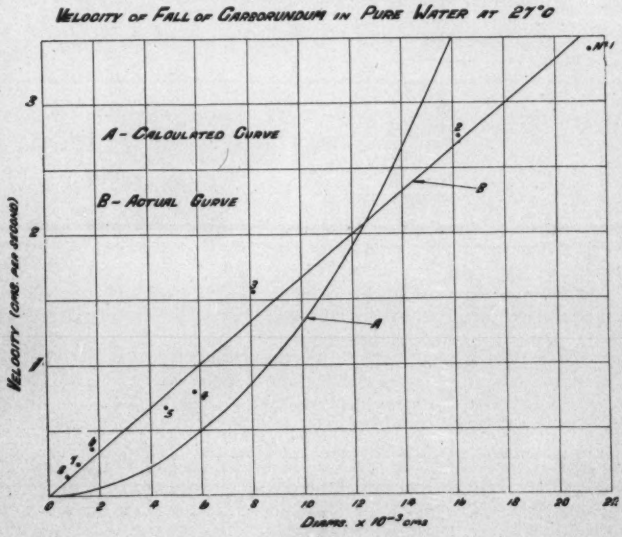


Fig. 3.

ordinates the velocities in centimetres per second. Curve B shows the result of the experiments made with carborundum, the particles of which are far from spherical and have irregular surfaces. It will be seen that the eight grades tested lie approximately upon a straight line, but there are evidences of a critical point, which is more marked in a similar emery curve. The coarsest grade tested was ordinary 80-mesh carborundum, and the finest grade marked on the diagram was much finer, so far as diameter of grain is concerned, than anything obtainable on the market, and equivalent to commercial thirty-minute emery so far as fineness of surface produced is concerned.

Whereas according to the curve A the velocity of fall should theoretically be proportional to the square of the diameter of the particles, actually in accordance with curve B the velocity of fall is directly proportional to the diameter. Thus, adopting the elements of Stokes' formula, the velocity of fall of a carborundum particle over a wide range of diameters can be determined from the formula

$$\text{Vel.} = \frac{1}{735} \frac{\text{R}(S_1 - S_2)g}{n} \qquad (6)$$

the value of n being for water at 27° C.

Assuming that it is desired to obtain along the bottom of the tank a uniform spacing of grades whose diameters are in simple arithmetical progression, and assuming that the grading tank is of uniform depth, it would be necessary, if Stokes' law were applicable, to increase the width of the tank uniformly in the direction of flow.

On the basis of Stokes' formula the plan of the grading tank should be triangular, but on the basis of the formula derived from the experiments described above, the tank should be parallel-sided, not triangular. It may be desirable in practice to increase the tank section towards the end so as to spread out the finest grades, but to do so is not essential.

As an example, suppose it is desired to find the length of bottom compartment that will catch the 80-mesh carborundum when the depth of the parallel-sided tank is 50 cms. and when the velocity of flow across the whole section is 3.5 cms. per second. The approximate radius of grain of what is commercially called 80-mesh carborundum is 0.0107 cm. It follows then from the equation

$$\text{L} = \frac{735\, v\, h\, n}{\text{R}(S_1 - S_2)g} \qquad (7)$$

that L, the length of the compartment, is equal to 50 cms. For grains of half the size the length would be 100 cms., and so on.

In the case of a simple parallel tank such as is indicated in Fig. 4*a*, particles of diameter d will settle along the path AD, provided the horizontal velocity of the water is constant at all depths from the surface A to the top of the receptacles B. Similarly, particles having diameters $\frac{1}{2}d$ and $\frac{1}{3}d$ will follow the paths AE and AF.

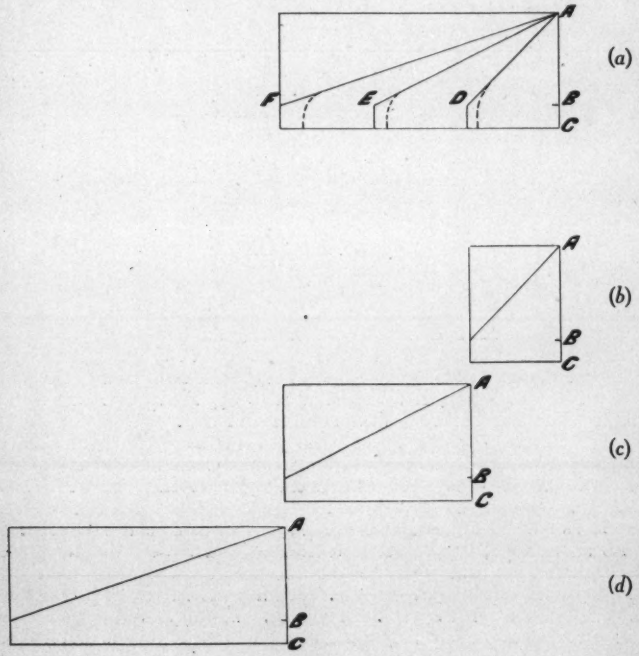


FIG. 4.

In practice it is not possible to maintain a constant horizontal velocity at all depths. There will generally be a decrease of velocity towards the bottom, the portion BC being dead water. The path of the particles will accordingly become curved downwards. Thus the spacing of the

finest particles will be appreciably reduced. For this reason, and to increase the spacing, a reduction of the velocity of flow in the later stages is actually desirable.

Instead of the single tank an equivalent series of tanks may be employed, as indicated in Figs. 4*b*, 4*c*, and 4*d*, in which for simplicity it is assumed that the horizontal flow is the same in all the tanks and constant throughout the depth. The unsettled material from the first tank passes to the second, and that from the second passes to the third. For the settling of the particles of diameters d, $\frac{1}{2}d$, and $\frac{1}{3}d$, the lengths of the tanks *b*, *c* and *d* must be equal to the lengths BD, BE, and BF of the first tank respectively.

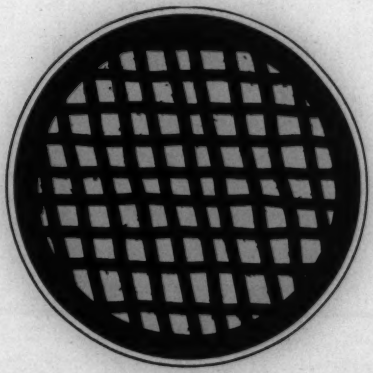


FIG. 5.—No. 120 SIEVE. MAGNIFICATION 27.

Frequently circular tanks are employed in series. It is not easy to suggest any good reasons for the use of such a form in preference to rectangular tanks. In a circular tank the flow across the sections must be complex and difficult to control. Even in a straight tank the prevention of eddy currents and the maintenance of a steady and uniform flow are problems of the greatest difficulty. In the case of the finer grades very slight eddy currents are sufficient to prevent settling of the particles.

Initial irregularities in the force of the stream may be destroyed by

making the water move down and up between vertical baffles and then pass onwards through a series of vertically arranged gauze screens, the function of which is to destroy eddy currents and not to retain any of the carborundum. The mesh should be selected accordingly.

At the entrance to each tank there should be provided screens that will retain any particles that should not have passed the preceding tank. Wire sieves suitable for the purpose having 230 holes and under per linear inch are obtainable, but for finer grades it is unfortunately necessary to depend upon silk sieves, the finest varieties of which are not readily obtainable from France at the present time. The choice of wire sieves

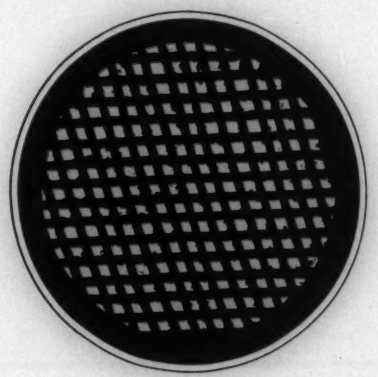


FIG. 6.—No. 220 SIEVE. MAGNIFICATION 27.

is comparatively limited, and the regularity of the sieves themselves is far from perfect.

Fig. 5, for example, is a photo-micrograph of a No. 120 sieve, that is, one that has 120 holes per linear inch. It will be seen that the regularity is far from satisfactory, and although the warp and weft wires are of the same gauge, there are 120 holes per inch in the one direction, and only 108 holes per inch in the other, a difference of 10 per cent.

Fig. 6 is a No. 220 sieve that actually has 189 holes per inch along the warp and 175 along the weft.

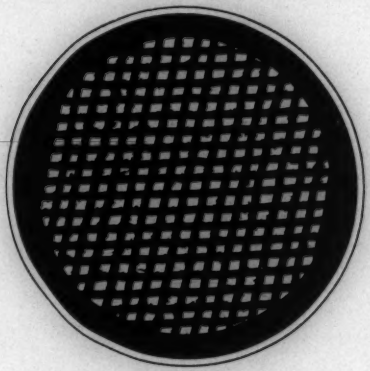


FIG. 7.—NO. 230 SIEVE. MAGNIFICATION 27.

Fig. 7 is a No. 230 sieve that actually has 216 and 189 holes per inch in the two directions, an average of 202.

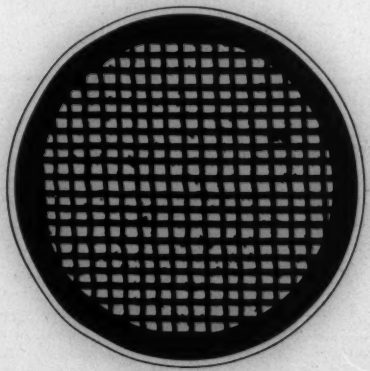


FIG. 8.—NO. 250 SIEVE. MAGNIFICATION 27.

Fig. 8 is a No. 250 sieve having 230 and 210 holes, an average of 220. In this specimen the warp wires are of heavier gauge than the weft wires. From a comparison of Figs. 7 and 8, it will be seen that although the sieve, Fig. 8, has the greater number of holes per linear inch, it is actually a coarser sieve than Fig. 7, owing to the wire of Fig. 7 being heavier than that of Fig. 8.

Fig. 9 is a photo-micrograph of a silk sieve in which the regularity of the spacing is obtained by using a double warp, the portions of which are twisted so as to hold the wefts securely in position.

To specify wire sieves by the number of holes per inch is not satisfactory, since, unless the wire gauge is also specified, it is not possible to determine the size of the holes or the opening factor upon which the

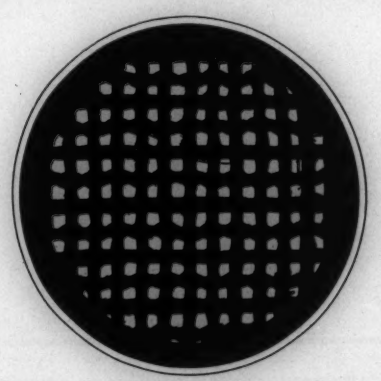


FIG. 9.—SILK SIEVE. MAGNIFICATION 27.

choking of the sieve depends. In specifying sieves the number of holes per inch might be denoted by the numerator of a fraction, and the standard wire gauge by the denominator; thus $\frac{100}{42}$ would be a sieve having 100 holes per inch and constructed of 42 SWG wire. The size of hole would be .006″ and the opening factor, that is the ratio of the width of hole to the pitch of the wires, would be 60 per cent. If the warp wires were 42 SWG and the weft wires were 40 SWG, the sieve could be indicated thus $\frac{100}{42,40}$. In this case the holes would be rectangular, not square, and the opening factor would be different in the two directions.

Neither of the above-mentioned sieves would give very satisfactory results, owing to the opening factors being too low. Speaking generally, the opening factor should be not less than 70 per cent. Choking of the sieve in practice becomes troublesome at 60 per cent., and a 50 per cent. sieve requires continuous attention.

For a given pitch of wires the opening factor increases as the diameter of the wires is reduced. But if the wires are too fine the sieve will not remain regular, and very slight pressure will separate them, thus allowing larger particles to pass. This effect is noticeable to some extent in most sieves, the force of a jet of water being often sufficient to alter the average size of the grains that pass, and it is very noticeable in silk sieves.

For the reasons above mentioned, it is not practicable at the present time to use a finer wire gauge than No. 46 SWG.

If N = number of holes per inch,
$\frac{1}{N}$ = pitch of wire,
d = diameter of wires,
D = width of sieve hole,
then $D = \frac{1}{N} - d$.

In Fig. 10 the abscissae represent the number of holes per inch plotted on a reciprocal scale, and the ordinates represent the widths of sieve holes or grain diameters. The thin oblique lines have been calculated in accordance with the above formula for the various SWG wire diameters. The 50, 60 and 70 per cent. opening factor lines are shown thickened.

Good commercial No. 80 mesh carborundum has an average grain diameter of about .0082″. This diameter is indicated by the thick horizontal line. All grades above this line are coarser than No. 80, and are, therefore, of no special interest. Of the sieve numbers that lie below this line, only a very small proportion are of practical use, owing to the opening factor requirements. All numbers lying below the specified factor line must accordingly be discarded.

Thus, suppose an opening factor of not less than 70 per cent. is desired, then the only usable wire gauges are 44 and 46 SWG. But the 44 SWG would only provide a very small range of from 88 to 94 holes per inch or .0082″ to .0074″ grain diameter. By using the 46 SWG an increased, but still very small, range is obtained, the smallest grain being .0055″, corresponding with $\frac{125 \text{ mesh}}{46 \text{ SWG}}$.

To obtain finer grades it is necessary to adopt a less satisfactory opening factor, which necessitates continual clearing of the sieve and a consequent forcing of larger particles through the mesh. For No. 8 carborundum, having a diameter of .0028″ and corresponding approximately with

No. 65 minute emery, it is necessary to use a sieve No. $\frac{200 \text{ mesh}}{46 \text{ SWG}}$ which has an opening factor of only about 50 per cent.

Considering the difficulties involved in the use of very fine silk sieves, even when the warps are very tightly stretched, it will be evident that the preparation of still finer grades of abrasive has already reached the practicable limit of the present day sieves.

The application of the principles already discussed to the design of a

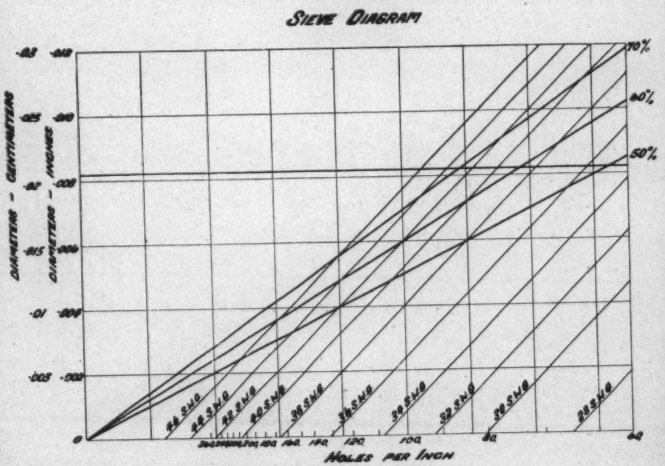


FIG. 10.

wet grading plant must necessarily depend upon the shop conditions. Only one typical arrangement that has proved generally satisfactory in use will be briefly described.

The plant consists of a series of tanks, one for each of the finer grades. One of these tanks is indicated in Fig. 11. The effluent from the preceding tank first passes through the sieve A, which is inclined at a small angle and is partially submerged. The water with the medium in suspension just laps over its surface. Heavier particles that should not have passed through the preceding tank are retained by this sieve, which also serves to separate airbells that may be attached to grains of abrasive. The

particles are carried down and up through the passages B and C and then onwards through the wire baffles D of comparatively open mesh. Heavy accidental particles may settle in the chamber below the passages B and C. The function of the wire baffles is solely to destroy eddy currents and to make the flow regular. The crossbar E penetrates the water surface and prevents the onward flow of surface particles that may be suspended by grease. After passing the wire baffles the particles settle in the portion F of the tank. In the portion G the water may be very gently agitated so as to make the particles rise and flow over the sieve H of the succeeding tank in which the sequence of operations is repeated in a finer degree.

Agitation of the water in the portion G may be effected in various ways. Very fine jets of water so disposed as to cause a whirl may be

TYPICAL GRADING TANK SECTION

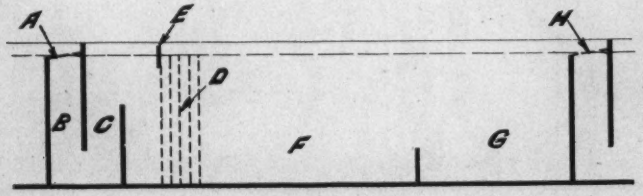


FIG. 11.

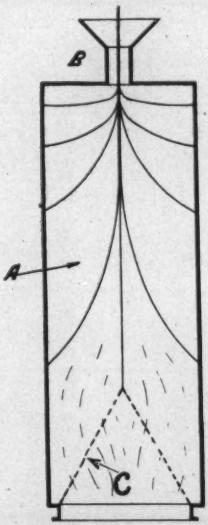
adopted or more natural upward circulation may be obtained by placing at the bottom of the compartment a small electric heating element. In some cases no circulating device is found to be necessary, the onward flow of the water being alone sufficient.

A plant of the type described is capable of grading carborundum to a fineness equivalent in effect to 35 minute levigation emery, the diameter of the particles being 0·007 mm.

Still finer grades have been obtained by the simple process of settling in dead water, and so far as carborundum is concerned no practical limit of settling has yet been observed.

By the application of centrifugal forces the rate of settling of the particles may be greatly increased, as compared with the natural rate of fall under the action of gravity in the case of a simple tank such as has

been described. The use of centrifugal machinery in chemical engineering processes is steadily increasing, and its application to the grading of abrasives has already been proposed.



Diagrammatic Section of Centrifugal Separator

Fig. 12.

In the diagrammatic section, Fig. 12, A is the cylinder which is rotated about its longitudinal axis. The usual centrifugal practice is to suspend and rotate the cylinder from the upper end B so that when the critical speed is reached, the mass will rotate quietly about its dynamic axis. From the bottom of the cylinder the effluent passes by gravity through a sieve of suitable mesh.

In the centrifugals designed by Mr. Gee and constructed by the Hydraulic Grading and Separating Company, of London, the sieve is so arranged, as indicated at C in the diagram, that any material that collects on the surface is thrown off by centrifugal force. This is a feature of much importance, because, if the sieves were placed oppositely around the periphery, they would become solidly blocked by such a material as carborundum or emery. In Mr. Gee's machines the cylinder wall upon which the material is thrown and consolidated is built up in vertical sections which can be removed for the separation of the material accumulated during the run.

Suppose that the chamber A is kept flooded. A particle will be acted upon by a radial centrifugal force proportional to the mass of the material with respect to water, the square of the angular velocity, and the distance from the axis of rotation. This radial force corresponds with the gravitational settling force of a tank, but its value will be very much greater.

In the vertical direction the fall of the particle will be determined by the action of gravity and the velocity of flow of the water, which should be kept constant. At high speeds of rotation the flow must be complex, but theoretically the particles will move along curved paths, as indicated

in Fig. 12, the coarser grades being grouped near the top and the finer ones spread out towards the bottom.

Considering the magnitude of the radial force as compared with the vertical, it is evident that in the case of heavy materials such as abrasives the spacing of the several grades in a single machine of reasonable size would be too small; and, further, since it is in the principle of the method that the coarser grades are situated above the finer, there would always be present the danger of coarse particles falling down over the fine zones when the machine was at rest.

As in the case of grading tanks, it would be necessary to use a series of machines, one for each grade or group of grades. This, however, means that the centrifugal grading machine, like the tank, depends upon the use of sieves, the fine limit of which has already been reached.

In conclusion, it would appear that grades of carborundum finer than No. 8 can best be obtained by natural settling in dead water tanks. Experience seems to indicate that much finer grades than No. 8 will settle naturally in a reasonable time, although the process is a slow one.

It is possible that electro-osmose methods when applied to carborundum may simplify the final settling process. Such methods are probably of interest only in the case of the last settling tank, where the whole of the residual material has to be precipitated from the water. It is still doubtful if electro-osmose methods will separate one grade from another with the certainty that is required for optical purposes.

DISCUSSION.

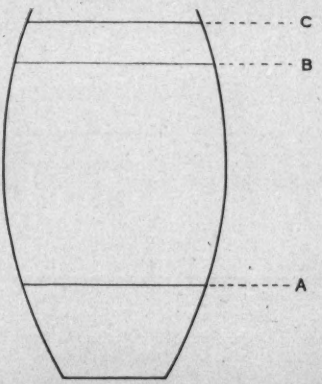
The PRESIDENT said that all must have been struck by the great practical value of the paper. This important research work which Mr. French had carried out primarily in the interests of his firm had been quite fully and freely disclosed in the paper which had just been read for the benefit of the industry generally. If the manufacturers present would follow the most excellent example set them by Mr. French, it would be of very great advantage to the industry, and, therefore, to the nation.

Mr. W. B. APPLETON said that Mr. GUILDFORD found it difficult to reconcile the present paper with previous ones by Mr. French, because the author appeared to have introduced a new idea in suggesting the possibility of polishing with a very fine carborundum. He (Mr. Appleton) thought, however, that Mr. French on a previous occasion had given them to understand that the grinding process which took place with the coarser material continued through the finer grades, whatever abrasive was used, and now he seemed to imply that, if one could obtain a sufficiently fine

grade of carborundum, it would produce a surface so fine as to become polished. It was interesting and helpful to hear what had been done at Glasgow in the way of research on abrasives, and to learn what difficulties had been met with, and the methods of overcoming them.

Mr. P. F. EVERITT said that the method of separation which depended on the fact that different sized particles required different times to fall through a standard depth of liquid could be subdivided into two alternative processes, the one, described by Mr. French, in which the larger particles are separated first and the smallest last, and another in which the opposite was the case. This latter process was, in his opinion, preferable for the separation of the finer grades of abrasive as required for "smoothing" glass.

A large earthenware pot with a smooth glazed interior, and shaped as shown in the diagram, is filled with the abrasive or sludge to the level A,



about one-quarter the height of the pot. Water is added till the pot is a little more than half full, and the mixture stirred and worked till the whole of the abrasive is completely suspended in the liquid; the pot is now filled up rapidly with water, stirred again and allowed to stand for, say, 20 minutes; at the end of this time the upper layer BC is poured off rapidly and as smoothly as possible, and after stirring well the pot is filled up and allowed to stand another 20 minutes. This process is continued until the layer BC, when poured off, is seen to consist of clear water.

By the repetition of stirring and pouring off the layer BC after standing

for 20 minutes, all the particles finer than the grade which will fall a distance BC in 20 minutes have been removed, as well as all particles floating on the surface either by air-bells or surface tension.

If, now, the whole process is repeated, the time being reduced from 20 to 10 minutes, all particles of sizes intermediate between those requiring 20 minutes to fall a distance BC and those requiring only 10 minutes will be extracted. Changing the receiver and altering the time to 5 minutes will give the next coarser grade, and so on. The abrasive is allowed to settle in the receivers and is stored in the usual manner.

It is essential to keep the depth of the layer BC poured off each time constant, at any rate for each grade, and also to cleanse the mouth of the pot carefully each time after stirring the contents.

In using this process of grading the abrasive all the grades finer than that in process of extraction at any given moment have already been removed; consequently the particles of the abrasive cannot carry down with them any much finer particles of another grade, as may happen in the method described by Mr. French. The result is that a very clean grade of abrasive is obtained.

The PRESIDENT asked Mr. Everitt whether he considered that the method he had just described would be capable of dealing with quantities such as Mr. French had to deal with, namely, 1 ton to 1½ tons per week.

Mr. EVERITT replied that the process as described was suitable for small quantities, but could easily be organised on a reasonably large scale as the quantity of smoothing abrasive required was always comparatively small.

Perhaps Mr. French would be good enough to prepare some corresponding grades of smoothing abrasive by the two processes and have a comparison made of their quality. A report of such tests would be of great interest.

Mr. F. G. MARSH said he had seen the apparatus described by Mr. Everitt, but had never seen it at work. It had taps down the side of the tank, which were used for allowing the water to run off within a certain time, in order to get the various grades of emery. He did not think it had been successful.

Mr. EVERITT said the method had not been used since 1912.

Mr. A. H. EMERSON said a somewhat similar method was used in Dalmeyer's works a considerable time ago. The method was to have a large barrel with a series of holes down the side fitted with pegs and then the water could at any given time be drawn off by drawing the pegs away.

Mr. EVERITT called attention to the contamination by suction, and said

that tipping over the top was far superior. If the tap were open near the top, some liquid would always be drawn from lower down, unless there was a very elaborate baffle plate.

Mr. MARSH said that Mr. French did not mention how he removed the particles of what he would call dirt from the fine grades of carborundum. He gathered that it was Mr. French's ultimate intention to carry the grading to such a fine point that it would be practicable to polish with carborundum. If that were the case, he thought trouble would arise from the fact that the specific gravity of the dirt particles, which might consist of a fine grit or grade of emery, or a fine grit of glass, such as British plate, might be the same, or practically the same, as that of the carborundum. It was his experience that a good "smooth" could not be obtained if such dirt particles were present in the finer grades of carborundum or emeries, although those particles were really soft. The dirt could not be removed by using sulphuric acid or hydrofluoric acid.

Mr. HENRY R. FAIRBANKS said that in the plant Mr. French had suggested he had an apparatus on the top of the tank for removing the scum, and he depended entirely on his sieves for the material to get through. While they were greatly indebted to Mr. French for his paper, he would certainly have liked him to have dealt rather more extensively with other abrasives besides carborundum or emery, such as corundum and aloxite. He himself preferred one of the other abrasives to carborundum when it came to fine work, because of the very great danger of a coarse grade of carborundum getting amongst the fine. But Mr. French's paper clearly showed that he was quite cognisant of that point. Emery was always more or less muddy and unsatisfactory. There was a great deal in the paper which would help those who had to deal with washing abrasives to grade them properly. Personally he would very much like to see something more extensive done with the centrifugal form of grading. Mr. French did not seem to favour that. He believed he was right in saying that a great deal of the porcelain and china made in England depended on their grading with the centrifugal method of depositing the grains; and he thought that with care and a little adaptation an optician would find the method very valuable indeed. It was quite true that the coarser grades came to the top instead of going to the bottom, but at the same time, with a long column of the abrasive rotating, there would be a good deal to pick from, and he thought with a matter of half-an-inch or so down the column of the abrasive, the grades could be split up in a very satisfactory way. The difficulty there obviously would be to have the abrasive very well screened before it went into the rotating tank.

Dr. WALTER ROSENHAIN said he was particularly interested in the

paper because a good many years ago, in a Lecture on Abrasives and Polishing before the Society, he had endeavoured to interest those concerned with glass grinding in the use of other abrasives than emery, which was at that time almost the only abrasive employed. His reason at that time was not at all based on any prevision of possible difficulties in getting emery, but entirely on the fact that he had found from his own experience, both in glass and other substances, the very much quicker cutting properties of carborundum and of certain other abrasives. For that reason he was particularly glad to see that Mr. French had done something to destroy what he could not help thinking had been a prejudice against carborundum, arising from that difficulty which Mr. French had fully recognised, and which the last speaker had also mentioned—the difficulty that a certain risk exists of coarser particles getting in amongst the finer ones. He was not sure that Mr. French had made the cause of it quite clear in his paper. The cause really lay, he thought, in the fact that the crystals of carborundum tended to cleave into flat flakes. No one, he thought, would have expected Stokes' law to apply to a flat flake falling through a liquid. If that were so, there would be no aeroplanes. The fact was, given a body whose horizontal dimensions were large compared to the weight and to the thickness, then there would be a degree of semi-stability in the air or in the water, which by suitable design and adjustment could be converted into actual stability. But what happened was that the substance fell much like a leaf or a piece of paper fluttering to the floor, taking a curious zigzag course, and then turning over eventually in precisely the way which Mr. French had described in his experiments. He did not think there was any question that this was characteristic of carborundum, and it had caused in the past trouble by the fact that coarse particles, which were large flat flakes, fell very much more slowly than they ought to do from their maximum dimensions, and therefore tended by the elutriation process to become mixed up with the finer grade. If that were carefully avoided—and the methods Mr. French had described did avoid it—then the difficulty, and the prejudice which had arisen from that difficulty, would be overcome, much to the profit of the optical industry.

There was one little practical difficulty which he would like to deal with quite briefly, and that was the question of air-bells. There was one method of removing air-bells which did not seem to have occurred to Mr. French; the whole of the air contained in the water could be expelled by boiling, and if the water with the abrasives stirred up in it were boiled, then the carrying over of particles by air-bells would be entirely eliminated. It would, of course, cost something to boil the water, but he thought very possibly it would save a good deal of trouble.

Then there were other points in that connection which were worth noticing, of which Mr. French was probably cognisant, but to which he

had not referred. He had drawn his own knowledge of them in the first instance from a paper by M. Henri le Chatelier, in an American publication called the *Metallographist*, but he thought it had appeared first in the French *Journal de la Societe pour l'Encouragement de l'Industrie*. In that paper M. le Chatelier had described the preparation of abrasives and essentially of polishing materials by the elutriation of a large variety of substances. He did not think carborundum was one of them. The writer dealt with alumina in various forms, and with iron oxide and chromium oxide, and he obtained very interesting and valuable results, particularly with the finest grades, by using not pure water, but water containing a little ammonia. It was adapted in the first place to retaining fine particles in suspension, and then altering the composition in order to produce precipitation. The presence of an electrolyte in water causes fine particles to become deposited very much more rapidly than they are in perfectly pure water or in the presence of non-electrolytes. Now, M. le Chatelier used water containing a very small amount of ammonia for suspending the particles, and that increased the suspending power of the water or decreased the rate of fall of very fine particles quite appreciably, showing in the first place that probably electrical and electro-chemical action played a part in this question besides the purely physical considerations involved in Stokes' formula. Then, having succeeded in obtaining these very fine suspensions in ammoniacal water, he added enough nitric acid to neutralise the ammonia, and immediately a rapid precipitation of the particles took place. That was certainly a very valuable thing when it was desired to obtain in a reasonable time the precipitation of particles which in the ammoniacal water would literally take weeks to come down. He (Dr. Rosenhain) had used it many times in polishing, and it certainly had valuable results.

The suggestion of the use of electro-osmose was a very important one. It was contained in Mr. French's paper, although he did not appear to have done very much with it. It should certainly be possible to deposit quickly by means of electrical methods.

Another practical point in Mr. French's paper was centrifugal precipitation. The difficulty which apparently had struck Mr. French most was that the coarser grains came to the top, and if anything fell out when the machine was stopped, the coarser grains fell among the fine. This difficulty could be got over entirely by forcing the water into the centrifuge from the bottom upward, thus reversing the whole process. The mechanical difficulty of forcing water into the bottom of a rapidly rotating vessel, of course, would arise, but that could be overcome.

The PRESIDENT said he ought perhaps to explain that the addition that Mr. French had made to his paper was evidently written when he got the first intimation that the Society were discussing that evening a paper by Lord Rayleigh on Polish. Some time ago Lord Rayleigh had

apparently been reading with great interest the proceedings of the Society, and he had written to him (the President) pointing out that possibly his paper on Polish, which was originally read before the Royal Institution in 1901, was not quite so well known as it might be, or perhaps should be. The Council accepted with pleasure Lord Rayleigh's suggestion that he should re-edit it and that it should be published in the *Transactions* of the Society. The paper was now ready for insertion in the *Transactions*, and he thought that perhaps the better course would be to accept it as read that evening. Of course, it was a well-known paper, and evidently Mr. French, having received a copy of it, had come to the conclusion that the Society were going to discuss the subject of Polish.

Dr. REGINALD CLAY said that in discussing this question it was too frequently forgotten that in addition to the motion of the particles through the liquid due to the combined effect of gravity and viscosity, one had to deal with the motions superimposed upon them by the stream line flow of the liquid, which resulted in a particle following a path not at all that shown in the diagram in Mr. French's paper. Mr. French no doubt quite well knew this, but it might not be equally well appreciated by everyone present. If water was supposed to enter a tank at the top left-hand corner and to flow across the central portion of the tank with uniform velocity, some particles of the water followed a straight line course along the surface, but the rest of the water had to distribute itself throughout the volume of the tank, and some portions of it actually flowed along near the bottom of the tank, so that the abrasive carried by the water would be carried through the tank at all levels quite independently of any effect due to gravity, and the fall to the bottom of the tank might be supposed to commence not only at the top of the tank but at every level. From this it resulted that the lines given in Mr. French's diagram were only the *limiting* positions of the particles. No particle of a particular size would be found above the line drawn by Mr. French, but it might be found anywhere below it. The result would be that particles of each size would be distributed all along the tank up to the point determined by the line drawn by Mr. French; there would, therefore, be at the entrance end of the tank particles of every size, at greater distances from this end first the larger particles would disappear, then those next in size, and so on, and ultimately only the smaller ones would remain.

Exactly the same thing applied in the case of a centrifugal apparatus, whether the particles entered at the top or at the bottom of the apparatus. Leaving out of consideration for a moment the centrifugal effect on the particles due to the difference between its weight and that of the liquid, owing to the stream line flow, particles of all sizes would be initially distributed at all distances from the axis, and particles which by this

flow were originally close to the surface would have a very short distance to travel under a very high radial force ; those which by the stream line flow were carried down along or near the axis would have a much larger distance to go and would start under a much smaller radial force. It had to be remembered that in a centrifugal apparatus the force with which a particle was acted upon was zero along the axis whatever the speed might be, and increased in proportion to the distance from the axis. Thus a very large particle which happened to be near the axis might get beyond the level at which it was expected to be thrown to the surface, just as on a joy wheel the people who were lucky enough to get into the centre might be able to stay on the whole time while those near the edge would be thrown off at once. This was probably the reason why the centrifugal method of separating abrasives had apparently been less successful than the tank method. (Dr. Rosenhain has suggested that this difficulty might very largely be met by putting a tube down the centre of the apparatus so as to limit the space in which the separation by centrifugal action was to occur, to one which was small in comparison with the radius.)

It seemed to him that the only scientific method of separation was to balance the natural fall of the particle owing to gravity by a corresponding vertical motion of the liquid. There were many ways in which the principle could be applied. Suppose a particle to be urged through the liquid with a velocity V either by gravity or in any other way, and that the liquid was moving in the opposite direction with the same velocity V, then that particle would remain at rest. Any particle which was slightly larger would have a higher velocity of its own which would overcome the opposite velocity of the liquid, and this particle would travel against the stream. A smaller particle in the same way would be carried with the stream. Thus all the particles which were less than the critical size would be separated from all those that were above that size ; and by a series of vessels in which the liquid moved with different velocities, or in which the particles were subject to different forces, an absolute separation into a series of graded sizes could be effected. The only difficulty might, as Dr. Rosenhain pointed out, be that owing to the flat shape of the larger carborundum particles such particles might travel at the same rate as smaller rounded ones. That might, however, be got over by suitable sieves.

A very simple way of producing the separation was to use a large inverted funnel and to send the stream carrying the abrasive into the bottom ; as it rose through the funnel the velocity would diminish owing to the increased cross section. Sieves would have to be put across the vessel both to produce the separation and to obtain a uniform flow.

These methods would not work on a small scale owing to the fact that the velocity in a small tube was zero at the walls and gradually increased

in the middle with a parabolic curve. This would cause small particles to be falling at the edges of the vessel at the same level as larger particles would be falling in the centre; but if the apparatus were on a sufficiently large scale the velocity curve would have a flat top in the centre of the vessel, and by using a "Guard Ring" method to carry off liquid from this central area where the velocity was uniform, particles of uniform grade could be obtained.

He would like to know how Mr. French did separate the glass from the carborundum, as he stated in one place that the specific gravity of the carborundum was almost exactly the same as that of glass. If so, he would have to use very close grading in order to effect the separation by sifting.

He would also like to know how Mr. French had measured the size of the particles in the experiment to which he had referred in which he obtained the straight line curve of Fig. 3; whether, for instance, he measured their diameters under the microscope, or whether he counted a certain number and weighed them and so obtained their average mass? The distinction was interesting in connection with the suggestion of Dr. Rosenhain that the straight line law was due to the non-spherical shape of the particles which caused them to disobey Stokes' law.

Mr. T. SMITH said it was interesting to find Dr. Rosenhain accepting Mr. French's experimental facts and supplying a theory for them; the two seemed to fit in very well. In comparing with Stokes' law the experimental law that Mr. French had obtained of the rate of falling of the carborundum flakes, there were two things to be noticed. In the first place, with large particles the flakes fell more slowly than they should do according to Stokes' law, and for that Dr. Rosenhain had suggested an explanation. The small fragments, though, fell much more rapidly than they should do according to Stokes' law. It might be that the two phenomena were to be explained on the same lines—that narrow planes tended at the start to fall with the plane vertical, and therefore moved more quickly than a sphere; but that the large ones set up a sufficient commotion (which might occur either at a definite velocity or at a velocity depending on the size of the fragment) for their motion to be somewhat disturbed, causing the plane to turn into a more horizontal position; a motion such as was indicated by Dr. Rosenhain would follow. Now, if that was the case, it meant that the particles, if round, would have followed Stokes' law, and the straight line law was obtained owing to their peculiar shape. It would be very easy to test whether that was the case, by experimenting with mixtures of flat flakes and round bodies; it might be that bodies sufficiently round for the purpose were the small fragments of glass mixed with the carborundum when it entered the tank. One would expect to find with such a mixture that when the grades were sorted out, the large flakes of the carborundum would be

mixed with finer pieces of glass, while the very fine pieces of carborundum would be practically smothered with larger pieces of glass, and so be useless for the purposes of grinding. It would be worth while examining what was deposited at different stages, to see whether the mixture was of such a character, and also to see whether the nature of the mixture changed with the length of time during which the deposit had taken place. If the correct explanation was on the lines suggested by Dr. Rosenhain, the relative quantities of the carborundum and the glass deposited in a short time should differ materially from those deposited over a considerable period. He thought it would be extremely interesting if Mr. French could say whether such results were found.

Mr. EMERSON said it was quite possible to exaggerate the point of the flakiness of the carborundum crystals. Mr. French had particularly mentioned them in the coarser grades only. They were produced in the mill in which they were broken up. The surfaces naturally followed the line of cleavage, and flatter flakes resulted. But the sludge from which the fine material was obtained was that produced in the rough grinding in the usual way, and this sludge consisted of particles of carborundum and glass which had been continually rolling over on the grinding tool, and he thought in the fine grades the grits were not so flaky, but approximated more to a regular shape than they did in the original crushed material.

Mr. S. D. CHALMERS thought Dr. Clay's remarks about the way in which the movement of the water affected the settling of the carborundum would apply also to his own case. The water coming up from below would give varying velocities to the different particles under the conditions Dr. Clay had described; he (Mr. Chalmers) thought that what happened in dealing with the grading of these particles was that they got a steady variation in grade, and it was not a matter of very great importance in dealing with, at any rate, some of the intermediate grades, if there should be some small variation in size. The variation in the cutting power of carborundum on its different faces and the different angles was considerably greater than would be caused by quite a perceptible difference in the actual size, and he had no doubt that Mr. French's apparatus did grade the carborundum sufficiently finely for practical purposes. But he had noticed that for polishing Mr. French used another method of obtaining these fine grades; that was to say, he allowed them to settle naturally in the water, and he thought that when it was necessary to have the grading very exact he had resorted to the method of simple direct settlement.

Another point was that possibly *some* of the polishing effect obtained with carborundum might be due to the method of treatment with hydrofluoric acid, and it would be very interesting to know whether the polishing

was to some extent influenced by the effect of this hydrofluoric acid upon the glass particles.

The PRESIDENT, in proposing a vote of thanks to the author, said that he knew beforehand the subject would prove an interesting one, and the discussion showed how important it was, in considering these questions, to have a sheet anchor in the form of a goodly company of practical workshop men present to lay the foundations of the discussion. Of course, the great advantage of having these questions discussed by practical men was that proper weight could be given to the various objections and arguments that were raised. The last time that he was in Glasgow, Mr. French had told him that he had almost succeeded in polishing glass without anything but water. In discussing the apparatus described by Mr. French it should be remembered that it was not in the experimental stage, but that, in principle at any rate, it had been in continuous and successful use at the firm of Messrs. Barr & Stroud for a long time.

He could not help feeling that in these discussions of Mr. French's papers the Society suffered under the very great disadvantage that the author was not himself present to enable them adequately to criticise his arguments.

Mr. F. W. PRESTON communicated the following remarks :—

It is stated in the paper, and was apparently taken for granted by the audience, that one substance is harder than another if it is capable of marking or scratching it. As a matter of fact all glasses will mark all others (vide " Jena Glass "). It seems to me that if the difference of hardness be not too great, the softer material will mark the harder, but when the difference is sufficient, no mark is perceptible ; the deformation is within the elastic limit. Thus a hard boro-silicate crown can be marked by a soft flint with which it is usually paired, and also by another piece of B.S.C. ; whence we should be obliged to conclude that this kind of glass is a good deal harder than itself. Thus the cutting of one material by another depends on the shapes of the parts in contact, *i.e.*, whether these are such as to cause local stresses beyond the elastic limit of the material. When the difference of hardness is considerable, as stated above, no mark is seen. Thus I have tried vainly to mark quartz with the finger nail, a bronze coin, and a steel knife blade, while the quartz will scratch the steel comfortably.

I think that, in accordance with these observations, the nature of glass grinding and polishing will be affected by the question whether the abrasive or polishing material is much harder or much softer than, or of approximately the same hardness as, the glass. If I am not mistaken, Mr. French in his earlier papers to the Optical Society advocates the view that polishing is merely a continuation of grinding, until the irregularities of the surface are small in comparison with a wave length

of visible light. This was Lord Rayleigh's view in 1901 (Proc. Roy. Inst. XVI., p. 563), but in 1905 (Proc. Opt. Convention) he is inclined to think that the process of polishing may be a molecular one, *i.e.*, that in a polished surface there may be no structure until we come to the molecular limit.

Now if the "grey" of the smoothed surface were sufficiently fine, there is no doubt that the surface would be polished. Hence, if carborundum could be made sufficiently fine, the structure left on the glass worked with it would be ultramicroscopic and the surface a polished one. It seems reasonable to suppose that such is the nature of a surface worked upon a fine grained material harder than itself. But ferric oxide is very much softer than quartz, and presumably incapable of cutting it (unless sleeks, which we regard as caused by impurities, are actually due to the rouge), and therefore, since quartz can be polished by rouge, it is natural, at any rate, to suppose that polishing by rouge is rather different in nature from polishing by carborundum. Moreover, the beginnings of a polish can be established by rubbing a finely-greyed surface with the hand, so that we may, I think, conclude that there is a polish due to molecular arrangement rather than to molar evenness of texture.

If, as Prof. Cheshire suggests, Mr. French is likely to be able to polish with nothing but water, this would rather tend to confirm Lord Rayleigh's later view.

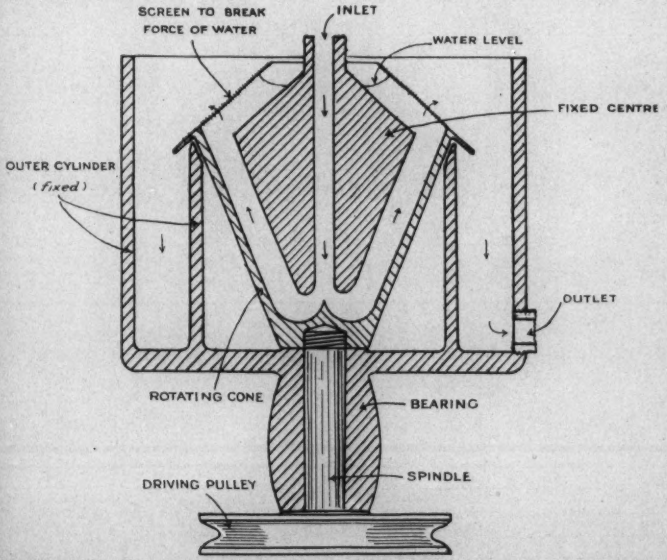
At the same time it is inconceivable that the molecular limit should be reached by carborundum which is not itself worked down to molecular dimensions. Hence there may very well be two distinct kinds of polish, unless the decisive factor in the operation is the backing material (pitch, wax, or cloth) and not the abrasive. We know that the backing material makes a difference: I have myself worked a lens with fine silica on a cast-iron tool, thereby producing a fine "smooth" and thereafter on wax with the same abrasive producing a polish. Moreover, rouge backed by cast-iron does not polish.

Personally I have come to think in view of some curious happenings in the workshop, that the process has very possibly an electrical nature. Pitch, wax and one's hand are all strongly negative to glass in respect of the development of frictional electricity; it would be interesting to try to polish with rubber, which is even more so. I am proposing to make some experiments with an electroscope as soon as possible.

As regards the air-bell trouble, and eliminating by boiling as suggested by Dr. Rosenhain, Mr. French is certainly aware of the possibility, since Mr. W. Taylor made the same suggestion to him long ago. It would be interesting to know if Mr. French has tried it.

The advantages of centrifugal grading—more rapid action and economy of space—appear to me such as to warrant its further consideration. The disadvantages named by Mr. French—that the coarse material is

at the top and therefore likely to fall and cause trouble, and that the grades 1-4 are much more closely packed than 4-8—can both be easily overcome. The slurry to be sorted can be introduced at the bottom. (Dr. Rosenhain mentioned this point, but there is no need for the rotating joint which he seemed to think necessary.) Then to keep the spacing of the joints more even we have only to alter the shape of the vessel from



cylindrical to conical. (Dr. Clay mentioned this.) To obviate a third difficulty, described by Dr. Clay—that a particle near the centre experiences little centrifugal force while it has a long way to go, whereas one near the edge having but a short distance to travel has a large force tending to move it—we may reduce the water way to an annulus or hollow cone. The above diagram illustrates the suggested improvements.

AUTHOR'S REPLY.

Mr. FRENCH writes:—I am glad this subject of the Grading of Carborundum has been so very fully discussed. To the manufacturer it is certainly a most important question which still deserves much consideration. If the difficulties referred to can be overcome and sufficiently fine grades of carborundum, emery, or kindred substances be produced on a commercial instead of a laboratory scale, there appears to be no reason why in the furure such substances should not be applied to the polishing operations.

I must apologise if I have done wrong in referring on this occasion to the subject of polishing, as appears to have been suggested. As our President explained, my remarks were submitted as a separate contribution to the proposed discussion on Polishing, which I understood was on the Agenda. But in any case I felt that the exhibition of specimens of glass polished with carborundum might serve a useful purpose in demonstrating one application of very fine grained abrasives, the production of which depends on the development of grading plants or other methods of separation.

In reply to Mr. W. B. Appleton, may I, in view of Mr. Guildford's difficulty, state very briefly some of the conclusions referred to in my previous papers on abrasives:

(1) In the forming and smoothing operations the glass is removed by a splintering action due to the pressure of the grain of abrasive.
(2) In the polishing operation, as explained by Lord Rayleigh and Sir George Beilby, the surface molecules are caused to flow or rearrange themselves in some way so as to form a perfect light-reflecting surface.
(3) From the earliest abrasion stage onwards there is observable some polishing action, all sharp edges becoming rounded.
(4) Up to the last polishing stage there is some removal of material arising mostly from the ploughing of the Beta layer.

In reply to Mr. P. F. Everitt, the method of settlement in dead water gives quite good results, but up to No. 8 carborundum grade the results are not noticeably better than those of the grading tank. For grades finer than No. 8 some settling method might be used with advantage, pending the development of a quicker and more certain method. There is one objection to the settlement method when operated on any considerable scale, namely, that it depends greatly on the care of the individual. Carelessness, say, in decanting too soon, or too quickly, can only be detected after the damage is done in the smoothing shop. In blocks

of large diameter that may be a very serious matter. The grading plant should be as fool-proof as possible.

Mr. Marsh has referred to details which I had cut out of my paper, fearing it was already much too long, The bulk of the iron is present in rather a peculiar form, at present the subject of special investigation. It appears to consist of strings of particles having a rounded or fused appearance, somewhat resembling the steel swarf from an emery grinding machine. Most of the iron is retained in the early stages, where its presence can do little harm. Any iron in the later stages could be dissolved out by acid, but this has not been found to be necessary in practice.

The presence of glass and the difficulty of separating it were at first thought to be matters of serious importance. Photo-micrographs of the sludge at all stages were compared and all were thought to contain more glass than carborundum. Further investigation showed that what was thought to be glass was really colourless pieces of carborundum. By analysis it was found that all grades contained some glass, but that while certain grades contained very little, others contained a considerable amount. Thus the first grades contain very little; Nos. 4 and 5 contain a considerable amount—as much as 20 per cent. has been recorded; Nos. 6 to 8 contain very little; and the great bulk of the glass lies beyond No. 8. The glass would appear to exist as fragments of fairly uniform No. 4 and 5 sizes and as very fine particles. At present the residue beyond No. 8 is discarded and for some time the stock of Nos. 4 and 5 grades accumulated. It was found, however, that notwithstanding the presence of so large a proportion of glass, the action of the Nos. 4 and 5 was very satisfactory. All grades are now utilised. During the past year the addition of new abrasives was only from 3 to 5 per cent.

I fear it will not be permissible, owing to the limits of space, to give particulars of the rates of abrasion for carborundum, glass, and other substances. Possibly I may have the opportunity later of dealing with the subject, which is one of much interest.

In reply to Mr. Henry Fairbanks, it would probably be wiser to use fine emery in preference to carborundum, if efficient grading appliances were not available. But once good grades of carborundum exist and are adopted, I do not think the operators on bonus or piece rates would welcome a return to emery. Even in the case of grinding wheels carborundum is being used in place of the various Al_2O_3 substances previously used.

With Mr. Fairbanks I agree that centrifugal methods have a future and I hope the attention of the makers will be directed towards meeting the requirements of opticians. In the case of China clay the grading is certainly very good, but for optical block work centrifugal grading by

stages would probably be imperative. If that is the case, the sieve becomes the limiting factor for the fine grades.

Dr. Rosenhain's explanation regarding the deviation of carborundum grains while falling is doubtless the correct one, but that Stokes' law would be fulfilled was hardly to be expected, considering the size of even the finest particles of carborundum, quite apart from their irregular form. So far as the grades in question are concerned, the form of the grains does not seem to be characteristically flaky. If that were the case the cutting properties of carborundum would not be so good as they are. Some of the grains, however, have an irregular spicule form and these are probably the ones that follow a zig-zag course.

With regard to the question of boiling the water to get rid of the air-bells, such a course, which is somewhat expensive and not very convenient, is fortunately unnecessary, as the air-bells part company with the grains at the meshes of the baffles and sieves.

With regard to the use of ammonia, such special methods may possibly have to be resorted to for the separation of the finest grades. Personally I have had no great success in the use of electrolytes, due probably to the elementary nature of my experiments. Research on the lines suggested is very desirable.

Dr. Clay's question regarding the separation of the glass has been partially dealt with in my reply to Mr. Marsh. In reply to the second question, the sizes of the particles were determined by measurement from photo-micrographs of the various grades. The figures are the means of a large number of measurements.

In reply to Mr. Smith, I do not think the carborundum grains are nearly so flaky as has been suggested, but further experiments on the lines suggested by Mr. Smith would be very helpful. In my reply to Mr. Marsh I have referred to the question of the distribution of the glass in the various grades.

Mr. Emerson's remarks, with which I generally agree, do not call for a reply.

In reply to Mr. S. D. Chalmers, there is no observable difference in the polishing action of carborundum treated with hydrofluoric acid and untreated. The carborundum with which the specimens exhibited were polished was thoroughly washed after treatment with hydrofluoric acid.

Mr. Preston seems to have misread my previous papers. The views attributed to me differ essentially from those expressed therein. As Mr. Preston suggests, "hardness" is a very indefinite expression. Speed, for example, plays an important part. That soft flint will mark hard crown is quite possible, if the conditions are suitable, but with regard to the experiment described, is Mr. Preston sure that the boro-silicate crown was harder than the flint? The reverse is sometimes the case.

If I understand Mr. Preston's argument rightly, the carborundum polish is different in quality from the rouge polish, on account of the different hardness properties of the two media. Perfect light-reflecting surfaces, however, can be produced with a great variety of polishing substances, such as glass, carborundum, emery, diamantine, P.P.P. powder, rouge, glassite, and even liquids. The hardnesses of these substances presumably vary greatly. Are we to assume there is an equal variety in the types of polished surfaces produced? A microscopic examination of a truly polished surface hardly conveys that impression. The carborundum polish is indistinguishable under the microscope from, say, a rouge polish, and when properly conducted a cloth-rouge polish, indistinguishable from a pitch-rouge polish, can be obtained.

Further, it is interesting to observe that the splinters formed in abrasion are generally of greater area than the abrasive grain itself, and yet the structureless surfaces exhibited were produced by grains of carborundum of quite visible size, by no means ultramicroscopic.

With regard to the sketch of the proposed Centrifugal Grading Plant, I think it is a mistake to depart from Mr. Gee's very nice central sieve arrangement, which is a feature of vital importance. In Mr. Preston's arrangement the sieves would become choked. They are not self-clearing. It is doubtful also if settlement would take place in the annular chamber, owing to the strong scouring action of the liquid.

Polish.*

By The Right Hon. LORD RAYLEIGH, O.M., F.R.S.

Presented 11th October, 1917.

THE lecture commenced with a description of a home-made spectroscope of considerable power. The lens, a plano-convex of 6 in. aperture and 22 ft. focus, received the rays from the slit, and finally returned them to a pure spectrum formed in the neighbourhood. The body of the prism was of lead; the faces, inclined at 70°, were of thick plate-glass cemented with glue and treacle. It was charged with bisulphide of carbon, of which the free surface (of small area) was raised above the operative part of the fluid. The prism was traversed twice, and the effective thickness was 5½ ins., so that the resolving power corresponded to 11 ins., or 28 cm., of CS_2. The liquid was stirred by a perforated triangular plate, nearly fitting the prism, which could be actuated by means of a thread within reach of the observer. The reflector was a *flat*, chemically silvered in front.†

So far as eye observations were concerned, the performance was satisfactory, falling but little short of theoretical perfection. The stirrer needed to be in almost constant operation, the definition usually beginning to fail within about 20 seconds after stopping the stirrer. But although the stirrer was quite successful in maintaining uniformity of temperature as regards *space*, *i.e.*, throughout the dispersing fluid, the temperature was usually somewhat rapidly variable with *time*, so that photographs, requiring more than a few seconds of exposure, showed inferiority. In this respect a grating is more manageable.

The lens and the faces of the prism were ground and polished (in 1893) upon a machine kindly presented by Dr. Common. The flat surfaces were tested with a spherometer, in which a movement of the central screw through $\frac{1}{100000}$ in. could usually be detected by the touch. The external surfaces of the prism faces were the only ones requiring accurate flatness. In polishing, the operation was not carried as far as would be

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* *From the Proceedings of the Royal Institution of Great Britain, 29th March*, 1901. 1917.—Recent discussions seem to recommend the reproduction of this lecture, though some of the conclusions may need qualification in view of the work of Beilby, French, and others.

† 1917.—This is the disposition often named after Littrow (1863). But it was effectively employed at an earlier date by Porro and Maxwell, from whom I learned it. The latter writes (Phil. Trans. 1860):—"The principle of reflecting light, so as to pass twice through the same prism, was employed by me in an instrument for combining colours made in 1856 and a reflecting instrument for observing the spectrum has been constructed by M. Porro."

expected of a professional optician. A few residual pittings, although they spoil the appearance of a surface, do not interfere with its performance, at least for many purposes.

In the process of grinding together two glass surfaces, the particles of emery, even the finest, appear to act by *pitting* the glasses, *i.e.*, by breaking out small fragments. In order to save time and loss of accuracy in the polishing, it is desirable to carry the grinding process as far as possible, using towards the close only the finest emery. The limit in this direction appears to depend upon the tendency of the glasses (6 in. diameter) to *seize*, when they approach too closely, but with a little care it is easy to attain such a fineness that a candle is seen reflected at an angle of incidence not exceeding 60°, measured as usual from the perpendicular.

The fineness necessary, in order that a surface may reflect and refract regularly without diffusion, viz., in order that it may appear *polished*, depends upon the wave-length of the light and upon the angle of incidence. At a grazing incidence all surfaces behave as if polished, and a surface which reflects red light pretty well may fail signally when tested with blue light at the same angle. If we consider incidences not too far removed from the perpendicular, the theory of gratings teaches that a regularly corrugated surface behaves as if absolutely plane, provided that the *wave-length* of the corrugations is less than the wave-length of the light, and this without regard to the *depth* of the corrugations. Experimental illustrations, drawn from the sister science of Acoustics, were given. The source was a bird-call from which issued vibration, having a wave-length of about 1·5 cm., and the percipient was a high-pressure sensitive flame. When the bird-call was turned away, the flame was silent, but it roared vigorously when the vibrations were reflected back upon it from a plate of glass. A second plate, upon which small pebbles had been glued so as to constitute an ideally rough surface, acted nearly as well, and so did a piece of tin plate suitably corrugated. In all these cases the reflection was *regular*, the flame becoming quiet when the plates were turned out of adjustment through a very small angle. In another method of experimenting the incidence was absolutely perpendicular, the flame being exposed to both the incident and the reflected waves. It is known that under these circumstances the flame remains quiescent at the *nodes* and flares most vigorously at the *loops*. As the reflector is drawn slowly back, the flame passes alternately through the nodes and loops, thus executing a cycle of changes as the reflector moves through *half* a wave-length. The effects observed were just the same whether the reflector were smooth or covered with pebbles, or whether the corrugated tin plate were substituted. All surfaces were smooth *enough* in relation to the wave-length of the vibration to give substantially a specular reflexion.

Finely-ground surfaces are still too coarse for perpendicular specular reflexion of the longest visible waves of light. Here the material may be metal, or glass silvered chemically on the face subsequently to the grinding. But experiment is not limited by the capabilities of the eye; and it seems certain that a finely ground surface would be smooth enough to reflect without sensible diffusion the longest waves, such as those found by Rubens to be nearly 100 times longer than the waves of red light. An experiment may be tried with radiation from a Leslie cube containing hot water, or from a Welsbach mantle (without a chimney). In the lecture the latter was employed, and it fell first at an angle of about 45° upon a finely ground flat glass silvered in front. By this preliminary reflection, the radiation was purified from waves other than those of considerable wave-length. The second reflection (also at 45°) was alternately from polished and finely ground silvered surfaces of the same size, so mounted as to permit the accurate substitution of the one for the other. The heating-power of the radiation thus twice reflected was tested with a thermopile in the usual manner. Repeated comparisons proved that the reflection from the ground surface was about ·76 of that from the polished surface, showing that the ground surface reflected the waves falling upon it with comparatively little diffusion. A slight rotation of any of the surfaces from their proper positions at once cut off the effect. It is probable that the device of submitting radiation to preliminary reflections from one or more merely ground surfaces might be found useful in experiments upon the longest waves.

In view of these phenomena we recognise that it is something of an accident that polishing processes, as distinct from grinding, are needed at all; and we may be tempted to infer that there is no essential difference between the operations. This appears to have been the opinion of Herschel,* whom we may regard as one of the first authorities on such a subject. But, although, perhaps, no sure conclusion can be demonstrated, the balance of evidence appears to point in the opposite direction. It is true that the same powders may be employed in both

* Enc. Met., Art. Light, p. 447, 1830: "The intensity and regularity of reflection at the external surface of a medium is found to depend not merely on the nature of the medium, but very essentially on the degree of smoothness and polish of its surface. But it may reasonably be asked, how any regular reflection can take place on a surface polished by art, when we recollect that the process of polishing is, in fact, nothing more than grinding down large asperities into smaller ones by the use of hard gritty powders, which, whatever degree of mechanical comminution we may give them, are yet vast masses, in comparison with the ultimate molecules of matter, and their action can only be considered as an irregular tearing up by the roots of every projection that may occur in the surface. So that, in fact, a surface artificially polished must bear somewhat of the same kind of relation to the surface of a liquid, or a crystal, that a ploughed field does to the most delicately polished mirror, the work of human hands."

cases. In one experiment a glass surface was polished with the same emery as had been used effectively a little earlier in the grinding. The difference is in the character of the backing. In grinding, the emery is backed by a hard surface, *e.g.*, of glass, while during the polishing the powder (mostly rouge in these experiments) is imbedded in a comparatively yielding substance, such as pitch.* Under these conditions, which preclude more than a moderate pressure, it seems probable that no pits are formed by the breaking out of fragments, but that the material is worn away (at first, of course, on the eminences) almost molecularly.

The progress of the operation is easily watched with a microscope, provided, say, with a ¼-in. object-glass. The first few minutes suffice to effect a very visible change. Under the microscope it is seen that little facets, parallel to the general plane of the surface, have been formed on all the more prominent eminences.† The facets, although at this stage but a very small fraction of the whole area, are adequate to give a sensible specular reflection, even at perpendicular incidence. On one occasion five minutes' polishing of a rather finely ground glass surface was enough to qualify it for the formation of interference bands, when brought into juxtaposition with another polished surface, the light being either white or from a soda flame; so that in this way an optical test can be applied almost before the polishing has begun.‡

As the polishing proceeds, the facets are seen under the microscope to increase both in number and in size, until they occupy much the larger part of the area. Somewhat later the parts as yet untouched by the polisher appear as pits, or spots, upon a surface otherwise invisible. Fig. 1 represents a photograph of a surface at this stage taken with the microscope. The completion of the process consists in rubbing away the whole surface down to the level of the deepest pits. The last part of the operation, while it occupies a great deal of time, and entails further risk of losing the "truth" of the surface, adds very little to the effective area, or to the intensity of the light regularly reflected or refracted.

Perhaps the most important fact taught by the microscope is that the polish of individual parts of the surface does not improve during the process. As soon as they can be observed at all, the facets appear absolutely structureless. In its subsequent action the polishing tool, bearing only upon the parts already polished, extends the boundary of these parts, but does not enhance their quality. Of course, the mere

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* 1917.—My experience in polishing was entirely with pitch.

† The interpretation is facilitated by a thin coating of aniline dye which attaches itself mainly to the hollows.

‡ With oblique incidence, as in Talbot's experiments (see Phil. Mag., XXVIII. p. 191, 1889), achromatic bands may be observed from a surface absolutely unpolished, but this disposition would not be favourable for testing purposes.

fact that no structure can be perceived does not of itself prove that pittings may not be taking place of a character too fine to be shown by a particular microscope or by any possible microscope. But so much discontinuity, as compared with the grinding action, has to be admitted in any case, that one is inevitably led to the conclusion that in all probability the operation is a molecular one, and that no coherent fragments containing a large number of molecules are broken out. If this be so, there would be much less difference than Herschel thought between the surfaces of a polished solid and of a liquid.

Several trials have been made to determine how much material is actually removed during the polishing of glass. In one experiment a

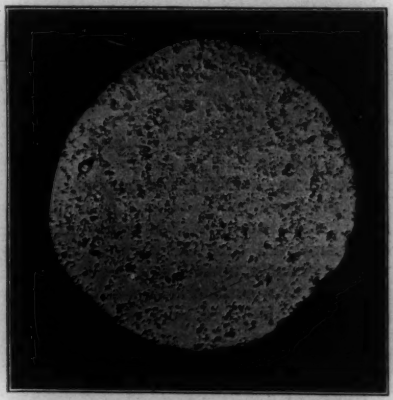

FIG. 1.

piece 6 in. in diameter, very finely ground, was carefully weighed at intervals during the process. Losses of ·070, ·032, ·045, ·026, ·032 gms. were successively registered, amounting in all to ·205 gms. Taking the specific gravity of the glass as 3, this corresponds to a thickness of $3{\cdot}6 \times 10^{-4}$ cm., or to about 6 wave-lengths of mean light, and it expresses the distance between the original *mean* surface and the final plane. But the polish of this glass, though sufficient for most practical purposes, was by no means perfect. Probably the 6 wave-lengths would have needed to be raised to 10 in order to satisfy a critical eye. It may be interesting to note for comparison that, in the grinding, one charge of

emery, such as had remained suspended in water for seven or eight minutes, removed a thickness of glass corresponding to 2 wave-lengths.

In other experiments the thickness removed in polishing was determined optically. A very finely ground disc was mounted in the lathe and polished locally in rings. Much care was needed to obtain the desired effect of a ring showing a continuously increasing polish from the edges inwards. To this end it was necessary to keep the polisher (a piece of wood covered with resin and rouge) in constant motion, otherwise a number of narrow grooves developed themselves.

The best ring was about $\frac{1}{2}$ in. wide. When brought into contact with a polished flat and examined at perpendicular incidence with light from a soda flame, the depression at its deepest part gave a displacement of three bands, corresponding to a depth of $1\frac{1}{2}\lambda$. On a casual inspection this central part appeared well polished, but examination under the microscope revealed a fair number of small pits. Further working increased the maximum depth to $2\frac{1}{2}\lambda$, when but very few pits remained. In this case, then, polish was effected during a lowering of the mean surface through 2 or 3 wave-lengths, but the grinding had been exceptionally fine.

It may be well to emphasise that the observations here recorded relate to a *hard* substance. In the polishing of a soft substance, such as copper, it is possible that material may be loosened from its original position without becoming detached. In such a case pits may be actually filled in, by which the operation would be much quickened. Nothing suggestive of this effect has been observed in experiments upon glass.

Another method of operating upon glass is by means of hydrofluoric acid. Contrary to what is generally supposed, this action is extremely regular, if proper precautions are taken. The acid should be weak, say one part of commercial acid to two hundred of water, and it should be kept in constant motion by a suitable rocking arrangement. The parts of the glass not intended to be eaten into are, as usual, protected with wax. The effect upon a polished flat surface is observed by the formation of Newton's rings with soda light. After perhaps three-quarters of an hour, the depression corresponds to half a band, *i.e.*, amounts to $\frac{1}{4}\lambda$, and it appears to be uniform over the whole surface exposed. Two pieces of plate glass, 3 in. square, and flat enough to come into fair contact all over, were painted with wax in parallel stripes, and submitted to the acid for such a time, previously ascertained, as would ensure an action upon the exposed parts of $\frac{1}{4}\lambda$. After removal of the wax, the two plates crossed and pressed into contact so as to develop the colours, say of the second order, exhibited a chess-board pattern. Where two uncorroded, or where two corroded parts, are in contact, the colours are nearly the same, but where a corroded and an uncorroded surface overlap, a strongly contrasted colour is developed. The combination

lends itself to lantern projection, and the pattern upon the screen (shown) is very beautiful, if proper precautions are taken to eliminate the white light reflected from the first and fourth surfaces of the plates.

In illustration of the action of hydrofluoric acid, photographs * were shown of interference bands as formed by soda-light between glass surfaces, one optically flat and the other ordinary plate, upon which a drop of dilute acid had been allowed to stand (Fig. 2). Truly plane surfaces would give bands straight, parallel, and equidistant.

Hydrofluoric acid has been employed with some success to correct ascertained errors in optical surfaces. But while improvements in actual optical performance have been effected, the general appearance of a

FIG. 2.

surface so treated is unprepossessing. The development of latent scratches has been described on a former occasion.†

A second obvious application of hydrofluoric acid has hitherto been less successful. If a suitable stopping could be found by which the deeper pits could be protected from the action, corrosion by acid could be used in substitution for a large part of the usual process of polishing.

In connection with experiments of this sort, trial was made of the

* The plates were sensitised in the laboratory with cyanine.

† Proc. Roy. Inst., March, 1893.

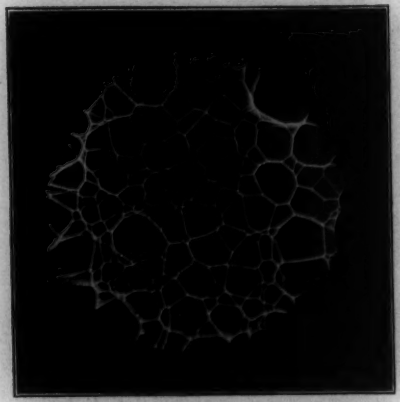

FIG. 3.

FIG. 4.

action of the acid upon finely ground glass, such for example as is used as a backing for stereoscopic transparencies, and very curious results were observed. For this purpose the acid may conveniently be used much stronger, say one part of commercial acid to 10 parts of water, and the action may be prolonged for hours or days. The general appearance of the glass after treatment is smoother and more translucent, but it is only under the microscope that the remarkable changes which the surface has undergone become intelligible. Fig. 3 is from a photograph taken in the microscope, the focus being upon the originally ground surface itself. The whole area is seen to be divided into cells. These cells increase as the action progresses, the smaller ones being, as it were, eaten up by the bigger. The division lines between the cells are *ridges*, raised above the general level, and when seen in good focus appear absolutely

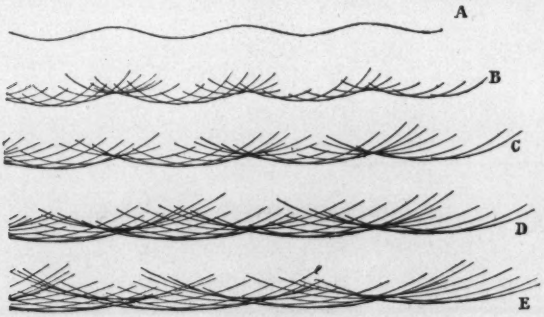

FIG. 5.

sharp. The general surface within the cells shows no structure, being as invisible as if highly polished.

That each cell is in fact a concave lens, forming a separate image of the source of light, is shown by slightly screwing out the object-glass. Fig. 4 was taken in this way from the same surface, the source of light being the flame of a paraffin lamp, in front of which was placed a cross cut from sheet-metal.

The movement required to pass from the ridge to the image of the source equal to the focal length f of the lens, may be utilised to determine the depth t of a cell. In one experiment the necessary movement was ·005 in. The semi-aperture y of the "lens" was ·0015 in., whence by the formula $y^2 = ft$, we find $t = ·00045$ in. This represents the depth of the cell, and it amounts to about 8 wave-lengths of yellow light.

The action of the acid seems to be readily explained if we make the very natural supposition that it eats in everywhere, at a fixed rate, normally to the actual surface. If the amount of the normal corrosion after a proposed time be known, the new surface can be constructed as the "envelope" of spheres having the radius in question and centres distributed over the old surface. Ultimately, the new surface becomes identified with a series of spherical segments having their centres at the deeper pits of the original surface. The construction is easily illustrated in the case of two dimensions. In Fig. 5 A is supposed to be the original surface; B, C, D, E surfaces formed by corrosion, being constructed by circles having their centres on A. In B the ridges are still somewhat rounded, but they become sharp in D and E. The general tendency is to sharpen elevations and to smooth off depressions.

ABSTRACT.

TWO LETTERS FROM PROFESSOR ABBE TO JOHN WARE STEPHENSON. (JOURN. ROY. MIC. SOC., April, 1917, p. 198.)

The following letters were communicated by Prof. F. J. CHESHIRE, and are inserted with the permission of the Royal Microscopical Society, by whom the blocks are lent.

I SEND herewith for the pages of the Journal of the Royal Microscopical Society copies of two letters, already of historical interest, relating to the design and production of the first Microscope homogeneous-immersion objective, made with the object, *inter alia*, of testing the possibility of obtaining enhanced resolving power by the use of such an immersion liquid. These letters, which were written by Professor Abbe to the English microscopist, John Ware Stephenson, towards the close of the year 1877, speak for themselves; I am able to bring them to the notice of the Royal Microscopical Society by the courtesy of their present owner. The first of these letters reads as follows:—

JENA, *Nov.* 14, 77.

DEAR SIR,

I beg your kind indulgence for the unpolite silence which, till now, I have opposed to your letters. I hope you will pardon me, considering that I am a much-occupied man. I wished to give a distinct answer to your question about oil-immersion objectives; and, being prevented from studying the question by want of time, writing has been protracted so long.

Now the matter is, that from a different point of view, some time ago I have considered already the oil-immersion. I had in view the advantages of this arrangement for petrographic work, *i.e.*, the observation of what we call "Dünnschliffe." As those thin plates of minerals, which are used for microscopic inspection, generally are roughly ground and not well polished, their observation with higher powers, even with immersion lenses, has some difficulty, which a fluid of higher refraction would prevent. In this aim I have made a preliminary calculation already in last winter; but I had not time to finish it. Your suggestion now induced me to study this object from a more general point of view, especially to take in consideration the increase of aperture, which this mode of immersion could possibly afford.

Some days ago I have succeeded in the calculation of $\frac{1}{8}$ ins. on this principle, which, I hope, will prove some advantage compared with the common immersion lenses. I have got a formula for the highest aperture, which, I think, has been realised till now, 112° *balsam*-angle—the product of refractive index, with the sine of the semi-aperture angle being = 1.25 (while with the immersion lenses of Zeiss till now this product is = 1.10). The technical work of such a system will be somewhat difficult, but I hope we will succeed. In a few days the lenses will be ground, and as soon as the thing is finished, I will send it to you and ask your judgment.

There is a difficulty in the combinations with oil-immersion, which will be observed as well in the technical work, *i.e.*, the fabrication, as in the application of such an objective, and which, perhaps, will make illusory this advantage, which you make prominent: the independence of cover-thickness and the dispensing of a correction-setting. The want of a medium of different refraction in front of the system will take away the most effective element for compensating the slight differences of workmanship in the lenses (the unavoidable differences of curvature and thickness) *and the differences in the length of tube*. By want of this compensating element the objective will be most sensible for the least difference in the length of tube; and, I presume, a sort of correction-setting will be necessary in this respect unless the use of the lens should not be restricted to a tube of exactly definite length. Now the experiment will soon settle this point.

As to the liquid, a mixture of oil of turpentine with oil of cloves will do very well, and it will be very easy to any observer to get the right mixture by observing the disappearance of a piece of cover-glass in the fluid, which is a very sensible proof for equal refractive index. . . . .

* * * * * * * *

Repeating my excuse for the long delay of my answer, I send you my best compliments, and remain

Yours most sincerely,
(Signed) E. ABBE.

The second letter reads as follows :—

JENA, *Decr.* 29, 77.

DEAR SIR,

I have the pleasure to give you notice, that the experiment with the oil-immersion objective has been successful. Some accidents in the work and a great pressure of business in Mr. Zeiss' workshop during the last months, have been the cause of a longer delay than I expected. But these difficulties have been overcome, we have got a lens of excellent performance. The aperture taken in my calculation—1.25 numerical value = 113° balsam-angle—has come out exactly without any lack of definition. The objective will bear extraordinarily deep eye-pieces, and in accordance with the calculated effects will give a very flat field. As I have no great experience in fine diatom work, I did not make many trials of this kind. But the advantage of the great aperture is already visible in a most striking manner on the image of *Pleurosigma angulatum* observed with oblique light. This diatom, mounted dry (but with frustules adhering to the cover) will give quite a new aspect if the oblique pencil of light is directed perpendicular to the axis of the valve (or, more generally expressed, *parallel* to any of the three ordinary sets of lines on it) : while rectangular

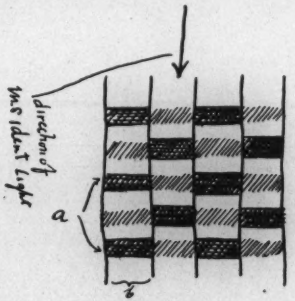

FIG. (A).

fields, separated by dark broad beams, alternating from row to row, *and these beams proceeding, as pale shadows, through the white fields of the neighbouring rows.* If *b* in the figure is the distance of the ordinary lines on *P. angulatum*, the interval *a* between the consecutive beams is

$$a : b = 2 : 2 : \sqrt{3}$$

This unusual image results from the entering of a new set of diffraction pencils, which by a smaller aperture are shut off. The diffraction spectre of any structure, the elements of which are arranged in congruent rows, crossing under angles of 120°, must have the following form: where the marks ● denote the six spectres next to the image A of the undiffracted

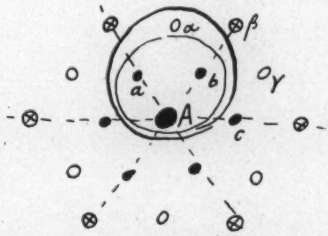

FIG. (B).

pencil, ◯ ⊕ the twelve spectres of a more distant circle. Now, in the case of *Pleurosigma* to an aperture of moderate amount, the spectres α, β, γ are inaccessible, even with oblique light, or at least so much weakened in their intensity, that no effect will get visible. An aperture of 1.25, on the contrary, admits, with oblique light, *one* of the spectres of the second circle with full intensity: in looking down into the open tube, the *four* pencils A, *a*, *b*, and α are to be seen within the margin of the system. This arrangement of light—four coherent pencils, arranged in a ◇—must give the image spoken of; and, especially, the coincidence of A and α must bring out a set of lines, parallel to the axis of the valves, the distance of which is to *b* as $1 : \sqrt{3}$. Those lines are to be seen *as continual ones throughout the whole valve*, if the two pencils *a* and *b* are moderated in their brightness, or quite stopped off by a diaphragm above the objective.

The managing of the system is quite easy. The length of tube held invariably there is no cover-correction necessary, indeed. Extremely thin and extremely thick covers give equal definition. There is only one inconvenience, which till now I did not master. I considered as unquestionable that among the great numbers of fluids, which are fit for use, there would be some, pure or mixed, which would give the exact refractive power of crown-glass and an equal dispersion too. But after having measured the refraction and dispersion of more than sixty oils and other fluids during the last weeks, I find that my assumption was premature; all the fluids, found till now, which have the refractive index of crown (or are brought to this refraction by mixture with another fluid) give a considerably higher dispersion. Therefore the different thickness of the fluid, caused by the difference of cover, must give a slight alteration of the chromatical correction of the lens.

If I should not succeed in finding a liquid of the right quality, the want will not at all be a grave one. In this case I shall adjust the colour-correction for *thin* cover, taking the liquid of minimum difference. For a considerably thicker cover, the correction now will be made perfect by adding to the liquid a small portion of a more dispersive fluid. This will not be a great inconvenience, I think.

In this way I have managed the system till now, taking the *oil of copaiva-balsam* as base of the mixture and adding varying portions of oil of cloves. Using a small bottle with

parallel faces (made of plate-glass) and a small prism of crown-glass plunged in, the refraction *and* dispersion of the mixture can be adjusted most exactly by simply viewing the window-bar through the prism and the liquid.

The provisory setting of the new objective would be somewhat troublesome to you, and besides that I cannot dispense with it in my further experiments with new liquids. From these reasons I have not yet sent it to you; I prefer to keep it until three new samples will be finished, which are in work now. The *best* of the four will be brought to your short setting, and will be sent to you as soon as ready, together with the testing-bottle described above. I hope you will agree with this delay. For petrographic work the oil-immersion will be exceedingly convenient; but besides that, as the greater aperture will yield a greater resolving power, lenses on this plan will be accepted by your English diatomists I think. But they will be much more costly as far as the unusual aperture is wanted—which will not be necessary for petrographic work. The workmanship is exceedingly delicate and difficult, especially as to the front lens. Among the twenty-four opticians in Mr. Z's workshop there is only one who can grind a front lens admitting a balsam angle of 113°, and only one who can make the brass-work to it. We were obliged to make several new contrivances for the work before it suceeded.

The sensibility of the system, in point of chromatical correction, for the dispersive power of the immersion-liquids, is an agreeable circumstance in one respect. By the nature of the optical glass, flint and crown, as it is till now, there is, unavoidably, a difference of spherical aberration, for different colours, and, from this cause, a difference of the colour-correction between the central and the marginal parts of a system. Now this difference can be easily regulated by slightly changing the dispersive power of the immersion-liquid; a smaller dispersive power of this liquid will give the best correction of colour for *oblique* light—a greater dispersion for central light.

In a similar way, slight differences in the length of tube may be perfectly compensated by a different mixture of the fluid.

In two or three weeks, I hope, the new lens will be in your hands; and I think you will be contented with the result, your impulsion has brought forth.

In the meantime I send you my best wishes for a happy New Year, and remain

Yours sincerely,

(Signed) E. ABBE.

The above letters, of course, are of very great interest to microscopists from the historical point of view, but they have in addition, at the present time, a peculiar interest for opticians and optical manufacturers generally, because of the information they give indirectly as to the high state of efficiency which had, even at that time, been reached at Jena in the work of optical designing and computing. We know that at this time Abbe had set himself 'to 'abolish once and for all workshop trial-and-error methods, and to replace the rough 'approximations, to the ray-path by more exact mathematical determinations, which, 'expressed in cast-iron formulæ, should enable the necessary structural elements of the 'lenses, such as thickness, diameters, radii of curvature, etc., to be exactly determined by 'calculation simply and alone.'

It will be noticed that on 14th November, 1877, the calculation for the proposed objective had been completed "some days ago." Six weeks later only, Professor Abbe was able to announce that his mathematically determined results had been realised in a concrete construction with great exactitude. We are told that the experiment with the oil-immersion objective had been successful; the computed aperture had been exactly realised without any lack of definition. This is a lesson which should be taken to heart at the present time.

Abstract.

Many people are under the impression that the designing and computing of first-class optical systems must of necessity be checked by workshop trials. By the methods of design introduced by Abbe the concrete embodiments of a system produced in a workshop do not test the conclusions arrived at by the designer and computor, so much as they serve as means for determining the accuracy with which the mathematical results can be realised in the workshop—*i.e.*, in first-class optical designing the designer tests the work of the workmen, the workman does not test the work of the designer.

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DESCRIPTION OF CERTAIN OPTICAL STORES WHICH HAVE BEEN CAPTURED FROM THE ENEMY.